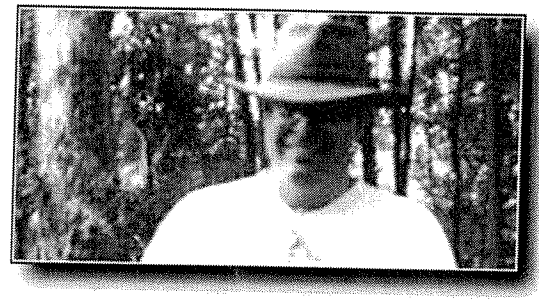

Robert J. Pierce, PWS

Born: April 28, 1948. Citizen of the United States. Married, two children.

Recognition:

Special Act Award, CECW-OR, Washington, D.C., 1989
Special Act Award, CECW-OR, Washington, D.C., 1987
Outstanding Performance Rating, DAEN-CWO-N, Washington, D.C.,1985-1986
Sustained Superior Performance, DAEN-CWO-N, Washington, D.C., 1983-1984
Outstanding Performance Rating, NADPL-R, New York, 1980
Chairman, Corps 404(b) (1) Testing Committee, 1978-1980
Outstanding Performance Rating, NADCO-OP, New York, 1977-1978
Sigma Xi Research Grant 1973
Sigma Xi, 1973

Education:

Legislative Operations Round Table For Executives, Washington, D.C., 1985
A/E Contracting for Military Construction, New York, 1982
Regulatory Functions Basic Course, Alexandria, Virginia, 1977

Ph.D., Miami University, 1977
Dissertation Title: Life History and Ecological Energetics of the Gizzard Shan (Dorosoma cepedianum), in Acton Lake, Ohio
Dissertation Advisor: Thomas E. Wissing
Doctoral Dissertation Fellow, 1974-1975
Doctoral Fellow 1973-1974

M.A., Miami University, 1972
Thesis Title: Energy Cost of Food Utilization in the Bluegill Sunfish (Lepomis macrochirus)
Thesis Advisor: Thomas E. Wissing
Graduate Summer Research Fellow 1971-1974

B.S., University of Dayton, 1970

Certifications:

Professional Wetland Scientist, 1995 Society of Wetland Scientists
Certified Wetland Delineator (Provisional), 1993 U.S. Army Corps of Engineers, Baltimore District
Maryland Forest Conservation Act, 1992 Maryland Department of Natural Resources

Professional Experience:

President, Wetland Science Applications, Inc., 1989 - Present

President, Wetland Training Institute, Inc., 1989 - Present

Biologist, Office of the Chief of Engineers, Regulatory Branch, 1983-1989

Biologist, Corps of Engineers, Cold Regions Research & Engineering Laboratory, Summer 1981

Biologist, Corps of Engineers, Planning Division, North Atlantic Division, 1978-1983

Biologist, Corps of Engineers, Operations Branch, North Atlantic Division, 1975-1978

Doctoral Teaching Fellow, Miami University, 1971-1973

Research Assistant, Miami University, 1971-1972

Teaching Assistant, Miami University, 1970-1971

Biological Aide/Technician, Bureau of Commercial Fisheries, Auke Bay, Alaska, Summers of 1968 and 1969

Research Assistant, University of Dayton Research Institute, Ohio, Part-time, 1968-1970

Online Publications:

Technical Principles Related to Establishing the Limits of Jurisdiction for Section 404 of the Clean Water Act.

Publications:

Wetland Training Institute, Inc. 1997. Nationwide Permits Complete: Volume 1. R.J.

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The Science of Wetland Definition and Delineation. Testimony before the House Subcommittee on Environment, Committee on Science, Space and Technology. November, 1991.

No Net Loss of Wetlands. Testimony before the House Water Resources Subcommittee, Committee on Public Works and Transportation. March, 1990.

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Human health impacts from ocean disposal of contaminated dredge spoil. Testimony before the Committee on Merchant Marine and Fisheries, House of Representatives, Ninety-Sixth Congress. May, 1980.

Wetland Field Work Experience:

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Arkansas	New Jersey
Arizona	New Mexico
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Statement Presented - Joint Hearing on May 9, 2006

Before:

Senate, Natural Resources & Environmental Affairs Committee
House, Natural Resources, Great Lakes, Land Use, and Environment Committee

Preliminary Findings on MDEQ's Report on the Impacts of Beach Maintenance and Removal of Vegetation under Act 14 of 2003

By:
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Chairs Buckholz and Palsrock, Vice Chairs Patterson, Pavlov and Gillard and members of the Committees, thank you for the opportunity to address you today on the import topic of conservation of the Great Lakes, in particular, Lakes Huron and Michigan. "Conservation" is the appropriate term to use because it is defined as "wise use." Use of the lakes can be for many purposes – and those uses may be competing or conflicting. It is incumbent upon elected officials and resource managers to attempt to optimize the use of valuable resources in such a manner as to have the broadest benefit to society in perpetuity.

As my address indicates, I am not a resident of Michigan. Maryland is a goodly distance from Michigan As a brief introduction and means of establishing my *bone fide,* let me state that I was born in Cleveland, Ohio and raised in Euclid, Ohio – the first suburb east of Cleveland. Lake Erie was at the end of my street. Great Lakes' water is a part of me from my earliest years – our soure of drinking water was Lake Erie. As a youth I enjoyed the beaches, boating, fishing and swimming in the Lakes. As a teen I sailed its waters as far west as Port Huron, delivering a boat one year to participate in the Mackinac Race. As an undergraduate at the University of Dayton, I conducted respiratory metabolism studies on juvenile coho salmon provided by the ODNR and destined for release in Lake Erie. I conducted my master's research on bluegill sunfish and my doctoral research was a life history and bioenergetics study of gizzard shad – both at Miami University. I was employed by the Corps of Engineers (COE) for almost 15 years during seven of which I was the technical monitor for three research programs – two dealing with contaminated dredged material and one with wetlands. The wetlands research program had two major thrusts - delineation and functional assessment. While employed at COE Headquarters, I also was the principal technical proponent for the COE wetland, training program. Since resigning from the COE, I have been a consultant working in wetlands and as President of the Wetland Training Institute, Inc. conducting training classes in wetland delineation, functional assessment, hydric soils, wetland hydrology, plant identification, policy, permitting and bioengineering and erosion control. I have done intensive study of wetlands in Michigan and have taught in numerous wetland courses in the State.

I was first contacted on April 11, 2006, by Save Our Shores and asked if I could be retained to technically review a number of documents related to beach management in Michigan. Since then, due to prior commitments, I have spent six days in Memphis, two days in Delaware and five days in New Hampshire. My review to date has not been as thorough as I would wish it to be, however, I have been able to delve into the documents and begin an assessment. My hope is that you will provide us time to conduct and prepare a thorough review of the materials before reaching any decisions.

As I see it, the fundamental question is will various forms of beach management have unacceptable, long-term effects on the Lake Erie, Lake Huron and Lake Michigan ecosystems? In terms more specific to your deliberations, does wise use dictate that simplified permitting procedures to authorize beach management be reauthorized, modified or abandoned in 2007.

I approached the task of reviewing the studies from two directions. As I read them, I highlighted excerpts that raised questions in my mind related to the design, methods, analysis, interpretation and ramifications of the studies. The studies are fertile ground for such an endeavor. My preliminary review has generated over 300 questions. My preliminary assessment is that neither of the studies provides an adequate basis for making a decision on the fundamental question.

My second approach was to ask myself how I would have designed and implemented a study to answer the fundamental question. Let me address, first, the preliminary review of the studies that have been prepared and then how I would have approached the question.

Perspective

I do not see how the Michigan Legislature can make a reasoned decision on the fundamental question without someone placing the beach maintenance activities into perspective. By perspective, I mean a discussion of the magnitude of the activities in relation to the overall ecosystem. Perhaps the Studies by Albert (2005) and Uzarski and Burton (200?) were not the appropriate venue, although I think that their findings cannot be extrapolated to a realm larger than the individual sites they sampled without the proper perspective. Certainly if it was not their responsibility, then it must be the responsibility of the Director of the Michigan Department of Environmental Quality (MDEQ). However, I find no real discussion of perspective in the *Report on the impacts of beach maintenance and removal of vegetation under Act 14 of 2003* (MDEQ march 2006).

The Great Lakes are immense. Lakes Huron and Michigan cover more than 45,000 square mile of surface area and have a combined volume of over 2000 cubic miles of water. The variation in climatic conditions from north to south also is large. Lake Michigan has over 1,600 miles of shoreline, which it shares with other states. Lake Huron has over 3,800 miles of shoreline, which is shared with Canada. Because the pilot program and reports do so, I will concentrate on Lake Huron, primarily, and Saginaw

Bay in particular although the discussion applies equally to the remainder of Huron and the other lakes.

Saginaw Bay itself covers 1,143 mi^2 and has 240 miles of shoreline – that's 1,267,200 linear feet. The watershed for Saginaw Bay covers 8,700 mi^2 – 15 percent of Michigan in 22 Counties. It is estimated that there are 15,000 acres of wetland in the watershed (4 percent) and, 175 inland lakes and ~7,000 miles of rivers and streams. Approximately 1.4 million people live in the watershed and there are approximately 1 million visitors annually to the four state parks located on the Bay. Clearly beach activities are an important use of the coast. Maintenance of beaches is not free. There is a cost to landowners and land managers so it is not likely to be conducted with out a substantial reason.

There are a lot of very important facts about Saginaw Bay that no one has presented. What percentage of the 240-mile coastline is wetland at low, median or high water levels? How many acres of coastal wetlands exist at low, median and high water levels? What is the water level below which wetlands become a problem to landowners and managers? How many years in the long-term cycle does maintenance have to be implemented? Does maintenance occur every year or just some percentage representing the low-water years? How many miles (acres) of wetlands are impacted by mowing? How many miles (acres) of wetlands are impacted by raking? How many miles (acres) of wetlands are impacted by filling? How many miles (acres) of wetlands are impacted by grading? What percentage of these maintenance activities, are conducted by private landowners versus public land-managers?

From MDEQ (2006) I gleaned the information provided in Table 1. Assuming that each approval represented a 100-ft wide swath of vegetation. Since MDEQ (2006) indicated that some approvals were for applications that exceeded the limits and were issued, I could not give an absolute maximum length of coast that was impacted. MDEQ certainly would know this value, however.

Table 1. Saginaw Bay Director's Letter Approval for Vegetation Removal (MDEQ 2006)

Year	Requested	Approved	Denied/ Withdrawn	Maximum Total Length of Coast (feet)	Percent of Saginaw Bay Coast	Mean Lake Water Level (feet)
2003	15	13	2	1,300	0.10%	577.073
2004	46	37	9	>3,700	0.29+%	577.791
2005	18	18	0	>1,800	0.14+%	577.728

It is readily apparent that a very small percentage – less than one percent - of the coastline of Saginaw Bay was impacted during the three years of implementation of Act 14 of 2003. I don't know what percentage of the wetlands along the Saginaw Bay Coast were impacted, but I think that it is essential that the Legislature know to put the activities into perspective. I don't know what percentage of the vegetation removal, was done by private landowners and what percentage was done by State Park managers, but I think that it is essential that the Legislature know. I don't know whether these numbers represent all of the maintenance activity in Saginaw Bay, but I think that the Legislature needs to know. I don't know whether the decrease in activity observed in 2005 will continue if water levels continue to rise, but the Legislature needs to have some projections on it.

Of course this just relates to Saginaw Bay. The Legislature also needs to know how much of all of the coastlines are projected to be impacted in similar ways and during what periods in time. Much of the coastline of the Great Lakes is never vegetated with wetlands, so the overall magnitude of the analyses is greatly reduced. Nevertheless, for good government to represent the legitimate interests of all of the public and to provide for wise use of the Lakes resources, there should be more factual information developed to address the activities in perspective.

Habitat Fragmentation

Perhaps the reason that not much has been written on fragmentation of aquatic communities is because there are major differences between the affect of fragmentation on emergent, lake-edge plant communities than on terrestrial communities because of the presence of water. Most researchers would not consider a large tract of forest that is divided by a river to be fragmented - yet a substantial, potential barrier exists for many terrestrial organisms to cross. On the other hand, dividing the same forest by an equally wide road would be considered fragmentation, even though terrestrial animals theoretically could move across the pavement the same as moving on the natural land surface.

In aquatic systems, motile species are able to move three-dimensionally from one side of open water to another without appreciable disturbance from humans. The aquatic system is more three-dimensional than terrestrial systems because of the buoyancy provided by water. All but benthic infauna can move vertically with ease as well as horizontally. Of course some organisms may be more susceptible to predation during the transit, but so is the mammal swimming a river, which encounters a large, largemouth bass. A fish crossing through open water from one emergent marsh to another is substantially different than a salamander or frog crossing a road from one inland pond or marsh to another.

The Lake ecosystems are adapted to drastic changes in shoreline marsh extent and fragmentation simply by the fact that many marshes do not exist during years of higher water levels. The extent of anthropic marsh fragmentation needs to be placed into perspective with naturally occurring fragmentation.

Validity of Reference Sites

Aside from the lack of perspective, my single biggest question about the two studies is the technical validity of the reference sites. Albert (2005) indicates that the initial design called for contiguous reference and treatment (treatments include mowing, raking, Hand pulling and filling) sites. He goes on to indicate that the design had to be modified because permission could not be obtained for access to contiguous properties except in one instance. Thus, the norm for the study is that the reference sites are at some, unspecified and variable distance from the treatment sites.

Albert (2005) does present maps (Figures 1 to 3) depicting the locations of the reference and treatment sites although the scale of the maps indicates that the reference sites are in many cases miles separated from the treatment sites. On the entire western side of Saginaw Bay, there is only one reference site (Figure 1, "Natural") for comparison of all of the treatment sites. Figures 20 and 21 depict someone's concept of wetlands lost since 1800. No attribution of the maps is given, so the veracity of their content cannot be examined. Furthermore, the locations of sample sites are coded differently than used in the study itself. Although Albert (2005) does not appear to do any direct statistical analyses comparing a specific reference site to a specific treatment site, the validity of the comparisons in the whole is not tested either.

Treatment sites, obviously, could be selected based upon the occurrence of some activity in the past – be it mowing, raking, etc. There is no indication of the method employed to select reference sites. I assume because nothing was stated that it was done by visual determination that each site was "undisturbed" and the ability to gain access permission to sample it. This is not the best approach from the standpoint of reducing bias that could be conceived. If in fact this was the approach that was taken, then it was incumbent upon the investigator to test the comparability of the sites. Anyone who has walked the coast knows that minor variations in topography, bathymetry, orientation in relation to wind direct, currents and fetch all can have major influences on the character of the shore. Indeed the results of the transect analyses and sediment samples indicate that there is "a large amount of variability in these wetlands" (Albert 2005, p. 14).

While there are concerns about Selection process and comparability of sample sites in Albert (2005), at least we know within the scale of the maps, where they are. No maps nor other location information are provided by Uzarski and Burton (200?). Albert (2005, p. 5) informs us that sites different than he used were utilized for sampling fish and invertebrates. Uzarski and Burton (200?) indicate only that they "selected adjacent or nearby intact wetland habitat as a reference and sought permission to access that area." They make it clear that permission was not always granted. Thus, the reader of Uzarski and Burton (200?) are left to guess at the location and pairing of sites. By adjacent, I assume that they mean contiguous. I have no idea what they consider "nearby."

Unlike Albert (2005), Uzarski and Burton (200?) do compare reference to treatment directly in many of their analyses. Thus, comparability of reference and treatment sites is of greater import in isolating the effects of the treatment from all other sources of variation in the system. We may accept that contiguous sites will be as similar as possible in a landscape (although it is possible to have drastic differences in conditions over short distances). However, it must be demonstrated that "nearby" sites are truly representative of pretreatment conditions at the treatment sites. No evidence is provided that such comparison tests were conducted.

Sampling, Data Manipulation and Statistical Analyses

Although I have had training in statistics and have used statistical procedures in various studies, I am not a statistician. There are a number of aspects of the design, implementation and analyses of the sampling protocols that have raised questions in my mind and really need to be reviewed by a statistician. We have been in contact with one that I have worked with in the past, who will be able to work on these reports in the future, but due to prior commitments was unable to review the material before this hearing. What follows are discrepancies that are raised in my mind.

For both studies, I am concerned with the effect that the disproportionate numbers of sample sites in each category of treatment compared to numbers of reference sites had on the statistical analyses. Furthermore, there is no discussion of whether the primary authors did all of the sampling, or whether others, such as students, did some or most of it. If the later is the case, there should be a discussion of the level of training and supervision that was provided. Since sampling can be skewed, by having multiple individuals perform similar tasks at different sites, there is a need for an analysis of the consistency between multiple field investigators. This is especially true for such activities as sampling invertebrates with dip nets and estimating cover with a quadrat.

Albert (2005) indicates that three, quarter-meter quadrat samples were taken within the "inner and outer emergent zone" at each treatment and reference site to quantify plant diversity, dominance and cover. No definition of inner and outer emergent zones is provided and there is no indication that these two zones accounted for all the plant zonation that might have occurred on a site. No indication that these quadrats were positioned randomly is provided. No plant lists are provided so the reader cannot determine the abundance of graminoids and forbes. Ocular estimates of graminoids in a quarter-meter frame would have been very difficult and perhaps not the most accurate approach to quantifying plant diversity and dominance. Stem counts were conducted for bulrushes but apparently not for other graminoids (if present).

Elevation and vegetation transects perpendicular to the shoreline were also employed to determine "if there are different types of shoreline involved in the study." No indication of whether a randomized process was used to identify the start of each transect is given. Sample intervals along transects were 5 to 10 meters determined by the width of the wetland or shoreline segment. Why the variation? Did interval vary on the same transect? How was the interval used on each transect determined? Was the interval estimated or measured? Use of transects was a sound approach and perhaps point of initiation and intervals were selected using a statistically sound approach, however, the report does not convey needed information.

I did not find the results of the transect vegetation analyses in the report. Furthermore, there was some anecdotal information reported on some differences at a few sites between 2004 and 2005, but apparently no systematic approach was used to try to assess annual variation at the reference or treatment sites. This is an important consideration, especially since reference sites were often distant from treatment sites.

With regards to root sampling, it is unclear whether any form of randomization was employed to select the sample locations. The number of samples varied from site to site, with no explanation. It is not clear that root analyses were compared zone by zone as would be most appropriate, especially since the length of each zone appears to vary from site to site. In assigning weights of fine roots to each species, a proportionality ratio was established based upon the rhizome weight in the sample. While this might be a valid technique, its validity should have been tested. It is quite possible that the proportion of fine roots to rhizomes differs between species and/or substrates.

Uzarski and Burton (200?) used a variety of methods in the study and changed some of the methods between 2004 and 2005. Mid-study changes make it difficult to try to interpret data in a meaningful way. Randomization of sample locations (e.g., location of fyke nets, grid sectioning of the invertebrate sorting pans, etc.) was not adequately addressed in the report.

Correspondence analysis techniques were used by Uzarski and Burton (200?) for analyses juvenile and adult fish and invertebrates collected along transects. It is unclear whether the analyses included an actual statistical separation of results or was only used to array the data with all conclusions being subjectively derived by the authors. Furthermore, the figures presented are so congested that interpretation is very difficult.

"Because interwetland variability was substantial" (p. 13), Uzarski and Burton (200?) "standardized" the chemical physical data that they collected along transects by calculating the difference of each observation from the mean for the wetland from which the observation was obtained. The process they followed has inherent errors associated with it. In the first place, the absolute value of a parameter [e.g., dissolved oxygen (DO)] often is far more important than the level of change observed. As example, Table 2 presents a theoretical scenario of DO along 160-m long transects in two different wetlands. When standardized using the Uzarski and Burton (200?) approach, both standardized data sets are identical. However, the interpretation of effect on the water column is very different. DO-A ranges from 14 to 6 mg/l – adequate DO for even trout to thrive in. DO-B ranges from 8 to 0 mg/l. DO levels below 5 mg/l would be very stressful on trout.

Table 3 is a theoretical example of how data collection and analyses can give misleading results. Uzarski and Burton (200?) sampled chemical and physical factors, along with larval fish using light traps along two transects in a series of wetlands. The reference transect originated at the outer edge of the wetland and was directed straight towards the shore. The anthropic transect originate 50 m shoreward of the outer edge of the wetland and was oriented parallel to the shoreline such that it intersected the reference transect. Uzarski and Burton (200?) only sampled along the anthropic transect to the intersection with the reference transect. DO-A and DO-B in Table 3 are examples of the kind of data that they might have collected. By not collecting data along the anthropic transect for the full 160 m produced a regression slope difference that was possibly more artifact of analysis than reflective of the environment.

If DO-A (Table 3) truly reflects conditions in the undisturbed wetland, then all of the values along the Anthropic transect that are beyond the intersection should be the same as indicated in the "DO ANTHROP PROBABLE" column (Table 3). Recomputation of the Standard along the anthropic transect yields the standardized values in the right-most column (Table 3). A graphical depiction of the effect on the regression line slope is given in Figure 1. Note that the red line represents the regression analysis for data collected along transects of comparable length to the reference transect. Its slope is much more gradual than that of the blue line which represents data collected only along 80 m of the transect. Thus, all of the slope comparisons along transects that Uzarski and Burton (200?) rely upon in their discussion of the chemical and physical parameters are suspect and should be reexamined.

Table 2. Example of how data manipulation can mislead interpretation.

Distance (m)	DO-A (mg/l)	DO-A STANDARD (mg/l)	DO-B (mg/l)	DO-B STANDARD (mg/l)
0	14	4	8	4
20	13	3	7	3
40	12	2	6	2
60	11	1	5	1
80	10	0	4	0
100	9	-1	3	-1
120	8	-2	2	-2
140	7	-3	1	-3
160	6	-4	0	-4
Mean	10		4	

Table 3. Example of how data analyses can mislead interpretation.

Distance (m)	DO REFER (mg/l)	DO REFER STANDARD (mg/l)	DO ANTHROP (mg/l)	DO ANTHROP STANDARD (mg/l)	DO ANTHROP PROBABLE (mg/l)	DO ANTHROP PROBABLE STANDARD (mg/l)
0	14	4	14	2	14	2.888888889
20	13	3	13	1	13	1.888888889
40	12	2	12	0	12	0.888888889
60	11	1	11	-1	11	-0.111111111
80	10	0	10	-2	10	-1.111111111
100	9	-1			10	-1.111111111
120	8	-2			10	-1.111111111
140	7	-3			10	-1.111111111
160	6	-4			10	-1.111111111
Mean	10		12		11.11111111	

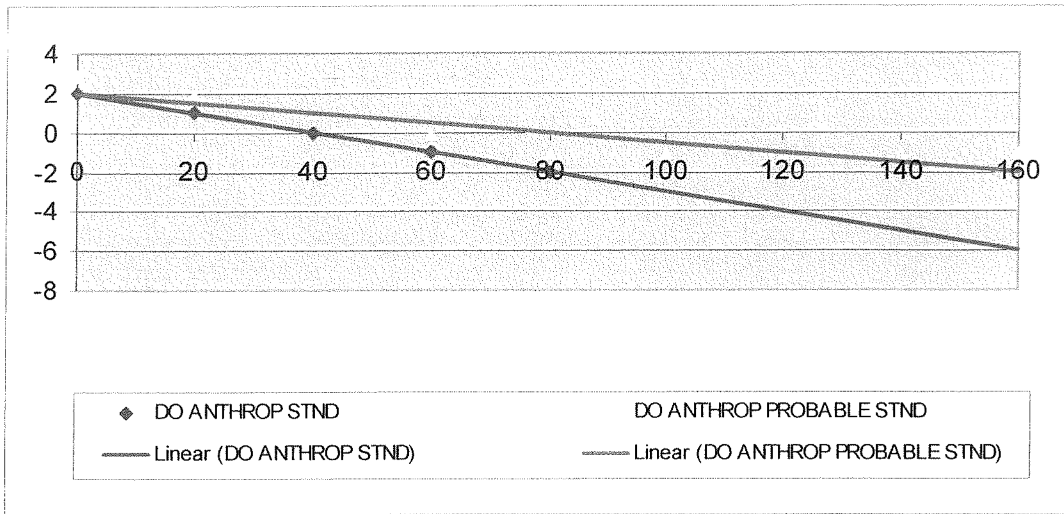

Figure 1. Misleading effect of data manipulation and analyses. The blue line represents data on the anthropic transect that was only collected to 80 meters. The red line presents the probable results if data were collected beyond the 80 meters to a full distance of 160 meters.

Misinterpretation of data is not related simply to manipulation and presentation. For example, the interpretation of data for larval smallmouth bass from anthropic transects by Uzarski and Burton (200?) is simply not consistent with the data as presented in their Figure 49 (reproduced below). They stated: "The anthropogenic transects reflected consistently low numbers with little variability among the four marshes included in the analysis. This consistency suggests a relatively large edge effect in these systems with respect to larval smallmouth bass." The data presented in Figure 49 show that there is no effect of the anthropic or reference edge of the wetland with regards to smallmouth bass larvae – there were no statistical differences. The number of larvae at 50 m shoreward of the open water edge along the entire anthropic transect is exactly consistent with the number of larvae at 50 m shoreward of the open water along the reference transect. The fact that the numbers of larvae are low along both transects may reflect the fact that smallmouth bass don't typically spawn in or near wetlands. The data could also indicate that depth of water (presumably the water depth 30 m from open water is deeper than 50 m from it) or some other factor is controlling the numbers of larvae and that the distance into the wetland is immaterial.

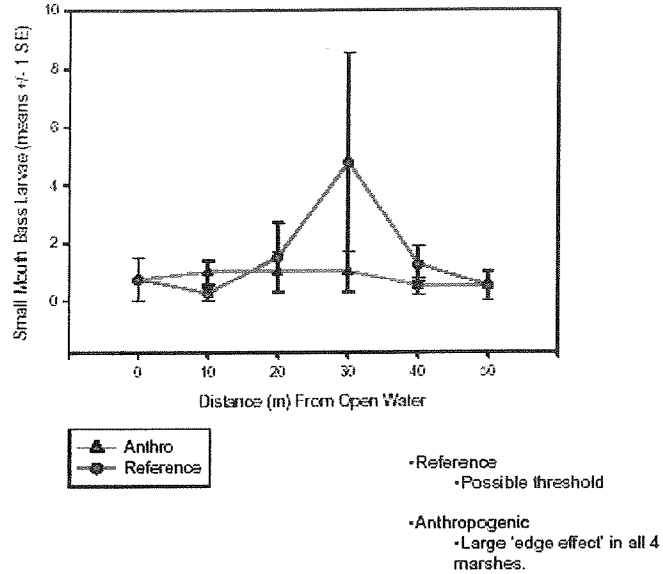

Figure 49. Larval small mouth bass abundances from open water into marsh fragments along anthropogenic and reference transects (Uzarski and Burton 200?).

For larval killifish, Uzarski and Burton (200?) found no statistically significant difference in number along the anthropic transect, but a statistically significant increase in numbers along the reference transect. Nevertheless, they concluded: "Variability … was relatively high on the anthropogenic edges suggesting, again, that the degree of hydrologic mixing controls the magnitude of the edge effect caused by vegetational removal" (p. 20). Examination of their Figure 51, however, reveals that there is much less variability along the anthropic edge than along the reference edge. Furthermore, the lack of statistically significant difference means that the killifish exhibited no edge effect on the Anthropic Transect and that the number found 50 m shoreward of the outer edge of the marsh was within the range of variability expected 50 m in along the reference transect.

The bottom line is that of the six species of fish that Uzarski and Burton (200?) chose to discuss in detail, only one, largemouth bass, showed an adverse anthropic edge effect when compared with the reference transects. Nevertheless, they concluded that: "Overall, larval fish communities appeared to be impacted by wetland fragmentation to spatial extents much greater than the immediate areas of vegetational removal" (p.21).

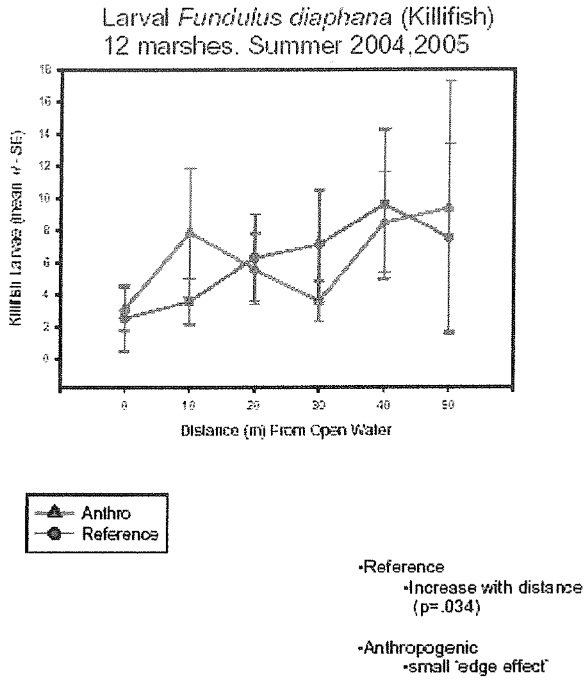

Figure 51. Larval banded killifish abundances from open water into marsh fragments along anthropogenic and reference transects.

Are All the Sites Wetlands?

Albert (2005) does not provide a listing of plant species found in each sample. Occasionally, he inserts the names of plants that have started growing in areas that have been raked or pulled in the past and not infrequently they are plants with a low probability of occurring in wetlands. If the water drops more than a foot relative to the land surface, and surficial, ground-water levels are controlled by lake levels, then the areas actually might not have been wetlands at the time of grooming – despite the fact that they were wetlands at higher water levels. This is not to suggest that regulation should bounce with predictable water cycles, however, the claim that wetlands are being lost due to the grooming practice must consider this fact.

Invasive species, such as *Phragmites*, often grow best were wetland hydrology doesn't exist or is at best marginal or temporary. Once established, they can easily spread far upslope from where wetland hydrology actually exists

Is Stagnation Good for the Lakes?

Uzarski and Burton (200?) at several places in the report seem to extol the concept that stagnation, anaerobiosis and perhaps even reducing conditions are a healthy and desirable condition for the water column in parts of the aquatic environment of Lake Huron and presumably the other Great Lakes. This concept is foreign to me. All of my experience, study and research always leads me to believe that healthy aquatic systems should remain aerated.

Their fascination with stagnant zones within the coastal marshes appears to be related to an overriding (and in my opinion misguided) concern that these coastal marshes serve as a mechanism for denitrification. They state: "Denitrification is the only mechanism for returning fixed nitrogen back to the atmosphere, completing the global nitrogen cycle," suggesting dire effects of a global nature if pelagic, oxygenated water intrudes into the near-shore marsh.

While denitrification was a hot topic with wetland scientists about a half dozen years ago, I almost never hear it discussed these days. When it was hot, I spent some time researching the topic in the literature and found out that while denitrification may be somewhat enhanced in some wetlands when compared with the rest of the landscape, the enhancement appears in general to be less than one order of magnitude in size. Probably the reason that it is not set forth with the same acclaim as it was. Furthermore, the types of wetlands where this small enhancement occurs appears to be those that are not wetted continuously, but only occasionally. This does not really characterize the near-shore, low-water, emergent marsh zone discussed by Uzarski and Burton (200?).

While much is made of the accumulation of greenhouse gasses in the atmosphere and the loss of the ozone layer, I am aware of no outcry that the level of nitrogen in the atmosphere is decreasing. The U.S. Fish and Wildlife Service has been telling us for decades that we have lost over half the wetlands in the lower 48 since Colonial times - more than a 100 million acres. Wetlands have been drained around the world for millennia, yet the percentage of nitrogen in our atmosphere continues to remain relatively constant. There is apparently enough buffering capacity in the world that nitrogen levels are safe. Personally, I'd opt for keeping the water column with as much oxygen as is possible and support a healthy faunal community.

Permitting Alternatives

I have spent my entire professional career working with aquatic regulatory programs – first in setting and interpreting permitting policies for the COE and later in teaching process and attempting to get permits for clients. I have also been involved on many situations of alleged violations of permitting requirements. I have observed that regulatory programs tend to become more complex and cumbersome as they mature – often without any additional benefit to the resource and usually with more cost and pain to the regulated public. For example, the COE Nationwide Permit Program, which authorizes activities with no more than minimal individual and cumulative adverse effects and is intended to reduce paperwork, has 27 general conditions (one of which has 18 subparts) while individual permits only have six general conditions.

MDEQ (March 2006) recommends that the letter of approval process be allowed to sunset and all subsequent actions require an individual permit. Personally, I think that this is a disservice to the public. Reduction of paperwork and simplification of process must be the goal of good government. Complexity and needless paperwork consume finite economic resources (both tax dollars and private funds), which are then unavailable for other purposes. As long as the final decision-maker retains the discretionary authority to require an individual permit or deny an activity when deemed necessary, then the general permit concept (letter of approval) is a cost-effective mechanism that serves the public's right to use resources while ensuring that the use is done wisely.

Retaining a general authorization process does not mean that the effects on the resource should be subsequently ignored. Periodic analyses of the effect of permitted activities should be a requirement for any program. These studies must, however, place the permitted activities within perspective to the resource being regulated.

My Approach

The Great Lakes have been studied for over a century, yet we still don't know what incremental effect most land management policies have on them. We do, however, have a great deal of existing data to draw on. The only way to access the long-term ramifications of removal of some areas of vegetation upon the health of the lake is to establish the natural level of variation that occurs with changing lake level. Because the changes are cyclic and do not necessarily establish long-tem stable water levels, the lake is undoubtedly adjusted to these fluctuations. If as suggested removal of vegetation is only necessary during short periods of low lake levels, then the net effect on the health of the ecosystem may be very minor. Thus, my first effort to answer the fundamental question would be to conduct detailed desktop analyses in several arenas. These certainly are not as much fun as traveling around the Lakes spending summer days on the beaches or water, but the fundamental question must be put into perspective.

Necessary first steps to put the issue into perspective include:

1. Determine what length of the shoreline would support vegetation absent anthropic management. This task is divided into subparts: vegetated coast during high water, vegetated coast during low water and vegetated coast during median water levels.
2. Determine what the width of wetlands is under each of the three water level conditions – length multiplied by width equaling area.
3. Determine what length of coastline is actively mowed under each water level condition and over what time period.
4. Determine what length of coastline is actively groomed (raked, pulled, filled) under each water level and over what time period.
5. Determine annual/monthly water levels in the lake.
6. Determine active, primary spawning grounds for various species of fish that rely upon vegetated areas for spawning. Some species such as banded killifish, carp, yellow perch and largemouth bass often use vegetated shallows for spawning grounds. Others such as Johnny darters, rainbow smelt and smallmouth bass, ascend tributaries or use unvegetated shallows and shoals for spawning.
7. Determine annual production levels of fish species

8. Toss in any other large database (e.g., waterfowl) that exists that relates to production from the Lakes.

The first four steps can be ascertained from intensive review of aerial photographs. I suspect that for most of the Lakes, there are aerial photographs available at least once in every 5 to 10 years dating back at least to the 1930's. Some areas may be more intensively documented than others. Steps one and two requires examination of the entire shoreline – not a trivial task – or establishment of a statistically sound, random sampling effort. For Lake Huron, the effort could be reduced by examining only the Michigan coastline and by assuming that the Canadian coastline is similarly configured. Further reduction could be done, by just examining one or two areas such as Saginaw and Traverse Bays. If aerials from a low water year where examined initially, then large areas of the coast could be eliminated from subsequent water level reviews because of the drowning effects of high water levels. The advantage of examining the entire shoreline in steps one and two is that much of the coast could be eliminated from consideration for future steps.

Once a good estimate of the extent and location of vegetated coastline under differing water levels has been established, then aerial photographs of randomly designed subsamples of vegetated shoreline can be examined for steps three and four. The information derived from aerial photographs can be supplemented by data from the permitting agencies. It is essential that the actual magnitude of the various beach management activities be estimated to make a reasoned decision on the impact that such activities will have on the Great Lakes.

Step five data is readily available on the World Wide Web. I think that the analyses of the aerial photographs must be done in concert with the water level data so that a simple, predictive model can be developed of how much coastal wetland can be expected under various water level conditions. Once this relationship is established, examination of the historical record of water levels will place the frequency and duration for the need to actively manage the beaches into perspective.

Finally, the wealth of fisheries and wildlife data can be examined in comparison to water levels and, thus, extrapolate to the role that coastal wetlands play on the resources of the Lakes.

Of course, many of these steps may have already been accomplished by some of the many researchers and resource managers that have worked on the Great Lake systems for decades. If they have been - great. The fact is though that these types of analyses have not been presented to the Michigan Legislature in these reports so that they can make reasoned decision based upon facts placed in perspective.

Preliminary Conclusions

As stated at the beginning, I have not had the opportunity to conduct a thorough analysis of the data presented. At this point in my evaluation, it appears that a lot of information that is available was not used to develop the MDEQ report and recommendation. The studies that have been presented, however, do not provide a convincing argument that the beach management activities under consideration cause any more than very localized impacts on the lake ecosystems – especially in the low numbers of activities that apparently occur. Having said that, it may be that after an adequate analysis of available data is prepared, a different conclusion would present itself.

If in fact, a fact which can be tested, the acreage of wetlands is greatly diminished during years with higher lake water levels as suggested in the Albert (2005) report, then the minor impacts associated with occasional, low lake water level, beach management is in all likelihood an imperceptible blip on the health chart of the lake ecosystems. Certainly, in all the years that biologists in Michigan have been studying the fisheries and waterfowl resources of the Great Lakes, someone must have thought to see how the variation in water levels affects the resource populations.

In the most-simple analysis, one needs only plot a measure of fish (or duck) abundance (e.g., commercial fish catches or angler success for species that may rely upon marshes for spawning) on the same graph with the long-term fluctuations in lake water levels. If an inverse, similar pattern exists (that is fish populations go down when water levels rise), then it is reasonable to conclude (although the conclusion could be wrong and the fish are responding to some other factor associated with high water levels) that the extent of marshes is an important factor and more refined analyses need to be done. Refinement would be to try and determine the critical water level and, therefore, extent of marsh, that is associated with healthy fish/duck populations. If no consistent pattern emerges (or the unlikely situation that the pattern is directly related to water levels, that is fish production is high when lake levels are high and wetlands are drowned out), then obviously the amount of coastal wetland is not a critical factor. Of course, there may be a lag time in the response of the faunal populations to water levels that must be considered.

Based on the Albert (2005) and Uzarski and Burton (200?) reports, it is appears that mowing strips of vegetation during low water levels consistent with the existing legislation has an essentially imperceptible effect on the coastal system. While the authors of the reports went beyond the results of the statistical analyses to postulate that repeated, long-term mowing may have deleterious effects, these assertions were not strongly founded and the fact that water levels will in all likelihood rise again and put an end to mowing seems to minimize the ramifications of those suppositions.

Grooming obviously has more direct and consequential impacts upon the immediate landscape that is groomed. Of course, a lot of the Great Lakes shoreline is not vegetated anyway – so the lack of vegetation in and of itself is not a drastic departure from norm. Aside from the minor chemical differences that were observed between the interior of near-shore marshes and the open water, water quality was not substantively adversely affected. I do no believe that reducing the zone of stagnation has any substantive adverse effects on the health of the Lakes as Uzarski and Burton (200?) apparently do. I disagree with the interpretation of Uzarski and Burton (200?) of their larval fish data. These data do not indicate that grooming causes substantial impacts to the intact marshes contiguous with the cleared swaths. At best, their data show an effect on largemouth bass larvae, and I do not see that the effect will have any substantive effect on largemouth bass production in the Lakes with the level of beach maintenance activity apparently being as low as it is. Their data show that for the larvae of five out of six species that they discussed in detail, there was no statistically demonstrable edge effect associated with the cleared swaths through the wetlands.

I have as yet been unable to really grasp the content of the Uzarski and Burton (200?) report concerning juvenile and adult fish. The analyses that they performed do not strike me as showing much effect, but I will need to have the statistician examine them before I can assess their import.

There were statistically significant lower numbers of macroinvertebrates in the water-column at groomed sites than at natural open water sites. Whether this condition would continue if the sites continue to be devegetated is unknown. It could be a temporary phenomenon associated with a lack of appropriate invertebrate species in the vicinity of the grooming or may represent a long-term condition. It would be interesting to reexamine the sites during high water conditions when the marshes have been drowned. However, based upon the limited extent of the grooming activities, I do not believe that even the long-term reduction in invertebrates in these limited areas would have an unacceptable adverse impact on the health of the Lakes. There were no data reported for benthic macroinvertebrates.

Neither of the reports addressed the direct loss of plant primary production associated with grooming as being a consequence that is important. Based upon the relatively minor amount of shoreline that is vegetated along the Great Lakes, especially during high water years, I think it is safe to say that coastal marshes are not the critical source of primary production for the Lakes. I am not advocating the loss of marsh, simply placing their role into perspective.

I was surprised that there was no mention in the reports of critical spawning grounds, although marshes do provide the primary location for spawning of a number of Great Lakes' fish species. If these marshes are important to the long-term viability of those species, I would have thought that spawning activity would have been one of the aspects of the study. If the wetlands actually exist only during years of very low water levels, then spawning fish may not be homing to them and they may not be used to any great extent. Of course with gamete production as large as it is in fish, the impact of these very small-extent grooming activities, probably will not have any detectable effect on the health of the ecosystem.

On the whole, my preliminary conclusion is that the data presented do not provide an adequate justification for intensifying regulation of the public at the level of activity that apparently exists. As I have said repeatedly, however, the analyses performed are in my opinion, inadequate to make a truly informed decision. The lack of any clear, substantive negative impacts beyond the actual localized and minor extent of the grooming activity, leads me to conclude that continuation of the general permitting mechanisms will not have an unacceptable adverse impact on the aquatic environment.